January 2, 1954

Dear Dr. Clowes:

Thank you for your air letter of the 23d.

I don't know where you got the idea that I was off to Europe next year. The opportunity would be delightful, but there are no prospects of it, and I cannot conceive how the rumor started? Like many others it is quite wrong.

Your other conclusion is more accurate. Our accomodations have been very limited (as Dr. Stocker, Spicer or Hayes might tell you); for a long time, we have been promised some amelioration, but there is no telling when the remodelling will actually be done. In that interval, we would be even more cramped; this coming year does not seem the most auspicious time for a visit; perhaps we can leave the question open for the future. If you do go elsewhere in the States, please do make an opportunity to visit us in transit.

It would take too long to go into here, but I wish I had an opportunity to discuss more intimately with you your last paper with Rowley on linkage. There are other ways of looking at F polarity and segregation besides Hayes' formulation; while for many aspects of data with haploid recombinants, it makes little difference whether the loss of genetic material from the F+ parent occurs at a pregametic or a postzygotic stage, studies on diploids have strongly supported the latter. Similarly, they are consistent only with a very precise delineation of the points of breakage (which are assumed to be the immediate forerunners of elimination). For example, the Lac locus (or Xyl or Ara_x) has never been eliminated (i.e. hemizygous in diploids) while Mal always has. Even more strikingly, Lp and Gal_1 are always eliminated; Gal_4 (extremely closely linked to these) never. As far as Cavalli and I could tell, your data were still interpretable on this basis, though, unfortunately, since the data were tabulated only one locus at a time, rather than in the more laborious, but more informative, pattern of a frequency list of all the pertinent combinations, it was hard to be sure.

Indeed, If it is not too impertinent, may I raise a suggestion? Your first letter implied that you were interested in broadening your genetic training. If you have in mind just these problems of formal analysis of K-12, I hardly know whom else in the U.S. I could recommend to you, that would not represent a rediluted version of Bill Hayes' own formulation, simply because this type of work has not been widely taken up in the US either. I would say, though, that Cavalli represents perhaps the most expert school in the formal analysis of recombination in bacteria. If you are planning to travel next year, had you thought of applying to Dr. Cavalli at Milan? As you may know, he was here for a short while, but is travelling through the country (or rather has just completed this) and should be back home almost as soon as you receive this letter. (I will forward the reprint you addressed to him here). Whatever you decide, I wish you a most profitable Wanderjahr.

As concerns my last paragraph but one, Jinks also might be able to give another side to the story.

Yours sincerely,